lore. The preparation of the work was intrusted to Mr. G. L. Gomme, the Director of the Society, who, it will be remembered, has recently published a very suggestive book entitled, "The Village Community," and is the author of numerous papers on folk-lore. Mr. Gomme has been assisted in the preparation of the book by several members of the Folk-Lore Society. At the outset Mr. Gomme defines "What folk-lore is," and after some judicious remarks he states: "From what has been advanced it may be conceded that the definition of the science of folk-lore, as the Society will in future study it, may be taken as follows, '*the comparison and identification of the survivals of archaic beliefs, customs, and traditions in modern ages.*'" The subject is divided into four sections, each of which is further subdivided. The principal divisions are as follows:—(1) Superstitious belief and practice: including superstitions connected with natural objects, goblindom, witchcraft, leechcraft, magic, beliefs relating to future life, &c. (2) Traditional customs: festival customs, ceremonial customs, games, local customs. (3) Traditional narratives: nursery tales, *Märchen*, fables, creation, deluge, &c., myths, ballads and songs, place legends. (4) Folk sayings: nursery rhymes, riddles, proverbs, nicknames, place rhymes. For each division there is a short account of the scope of that especial branch of folk-lore treated in a comparative manner, illustrations being drawn from the most varied sources; by this means the reader is enabled to gain a comprehensive view of the subject; then follow a number of questions for the guidance of collectors. The plan of the book is much the same as that of the well-known "Anthropological Notes and Queries," and it does for those interested in folk-lore what that estimable little work has done for the traveller—it instructs him what to do and how to do it. The author appeals not only to those who have the opportunity or the inclination to mix with "folk" (or "the less advanced classes in cultured nations"), but also to those who prefer to or possibly only can work in the library, and valuable hints are given to the latter, and two specimen tabulation forms are inserted. It is to be hoped that the publication of this carefully prepared hand-book will give definiteness of aim to the numerous people who take an interest in all "relics of an unrecorded past," and who would gladly collect information if they knew what was worth recording, how to arrange their facts, and where to send them to. Such information this book supplies, and the cost, being only half-a-crown, places it within reach of all.

Physikalische Krystallographie. By Dr. M. Liebisch, o.ö. Professor der Mineralogie an der Universität Göttingen. (Leipzig: Veit and Co., 1891.)

THIS book furnishes another example of the profound learning and patient industry of the German man of science. Dr. Liebisch, who is well known as one of the editors of the *Neues Jahrbuch für Mineralogie, Geologie, und Palaeontologie*, has recorded in the large octavo volume (of 614 pages) lying before us, a mass of investigations and abstruse mathematical proofs and deductions dealing with the physical properties of crystals. The subject is treated throughout in such a way as to bring out clearly the interesting relations existing between the physical phenomena and the geometrical symmetry of crystalline substances.

With this end in view, the author treats consecutively of homogeneous deformations, of the nature and orientation of isothermal planes, of thermo-electricity, of magnetic induction, of dielectrical polarization, and of pyro- and piëzo-electricity in crystals. The last part of the work deals with the elastic properties of crystals and with the remarkable applications of the theory of the elasticity of crystals on the changes in double refraction produced by pressure, concluding with an account of the investigations on the elastic deformation of dielectrical crystals in the electrical field and the electro-optic phenomena observable in piezo-electrical crystals.

Not the least valuable part of the book is the series of beautiful plates at the end of the volume, illustrating pyro-electrical phenomena and the phenomena of interference in doubly refractive crystals, the figures being prepared from photographs made by the author. Worthy of note is the fact that Dr. Liebisch has succeeded in photographing interference phenomena displayed in the monochromatic light of the sodium flame.

Valuable as the book is in its completeness and accuracy of information, there are few, we venture to think, who possess sufficient mental digestive power to assimilate its learned contents. Even the professed crystallographer will no doubt find within its pages some rather heavy reading.

Anleitung zur Darstellung chemischer Präparate: ein Leitfaden für den praktischen Unterricht in der anorganischen Chemie. By Dr. Hugo Erdmann. (Frankfurt-a.-M.: Verlag von H. Bechhold, 1891.)

A CONSIDERABLE evil of the present system of education is that the variety of subjects which a student has to pursue is increased, while the time at his disposal is, in many cases, less than it used to be. And as the examination is the only criterion of his success, both students and teachers are anxious to exclude all work that has not an immediate bearing upon this final test. In the study of chemistry, this state of affairs is leading to the exclusion of some of the most important branches of the subject, because practical examinations are invariably of an analytical character. When a student takes up organic chemistry, he is generally set to make a few preparations, and so accustom himself to work with larger quantities than a gram or two. But, although such practice is just as valuable and necessary to the student of inorganic chemistry, he very rarely enjoys a similar privilege. Dr. Erdmann has endeavoured to supply this deficiency so far as a guide book is concerned, and though in many cases his directions are rather meagre, he has put together an excellent series of instructions which refer to compounds of all the commoner elements. The word inorganic is not too literally interpreted, for we find that oxalic acid crystallized and dry, ethyl bromide, hydrocyanic acid, urea, thiophen, Prussian blue, and some acetates are included. The volume of 71 pages concludes with a few useful notes concerning apparatus and processes, and gives directions for obtaining streams of several of the commoner gases without the aid of heat.

Analysis of a Simple Salt. By William Briggs, F.C.S., and R. W. Stewart, B.Sc. Lond. (London: B. W. Clive and Co.)

IN compiling a guide, it is a good thing to go to the best sources for information, but it is a graceful thing to acknowledge whence the information has been gathered. We do not for a moment suggest that every paragraph should be encumbered with a statement of the various authorities that support the facts expressed in it; but when the manner and order of treatment, and very often the actual verbiage, are taken from a volume that is still in current use, it is certainly due to all who have an interest in that volume, that the compilers shall acknowledge their indebtedness. If the compilers had followed more closely still the scheme for the "preliminary examination in the dry way" as detailed in Valentin's "Qualitative Analysis," we think they would have improved their method. And it may also be remarked that, to prepare always neutral solutions in searching for acids, leads to much waste of time, and especially so when the substance for analysis is a simple salt. A few specimen analyses are added at the end of the book. These, even when judged of by the tables given in the body of the work, are not

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always complete, and sometimes inaccurate. Overlooking the points already referred to, the volume is likely to prove a useful and trustworthy assistance to those for whom it is especially intended.

Magnetism and Electricity. By J. Spencer, B.Sc. (Lond.), F.C.S. (London: Percival and Co., 1890.)

THIS work will form a useful class-book when the elementary parts of these subjects are being taught. It is divided into three parts, the first dealing with magnetism, while the other two treat of electricity, subdivided into frictional and voltaic electricity. The idea throughout has been to arrange a series of easy experiments, which the teacher or student can with little difficulty perform. In each case good instructions are given, and, following these, are the conclusions which can be derived from them. The scope of the subjects under treatment is limited to the elementary stage of the Science and Art Department, and at the conclusion of each chapter are arranged exercises, among which are some that have been previously set by that Department. In the appendix will be found the syllabus, and questions that have formed the subject of examinations of the Department during the last two years, which should be found useful to both teachers and students.

The Elements of Euclid. Books I. and II. By Horace Deighton, M.A. New Edition, Revised. (Cambridge: Deighton, Bell, and Co., 1891.)

THE chief alterations made in this new edition consist in the introduction of symbols and abbreviations; and in order that beginners should not be confused with them at the commencement, the first fifteen propositions are proved without their use, with the exception of the symbols for therefore, because, and equal to. For several of the exercises the author has substituted others which he considers more suitable, and, in addition, some new ones have been inserted. Of the two books dealt with, Book I. is treated very fully, and each proposition is followed by a considerable number of easy examples, which, if worked out by the student, should give him a thorough knowledge of the propositions preceding them. A most useful collection of propositions is added, with which the reader is recommended to make himself familiar.

LETTERS TO THE EDITOR.

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Mr. Wallace on Physiological Selection.

I WILL endeavour, as briefly as possible, to justify what I have said elsewhere touching "the peculiar position to which Mr. Wallace has eventually gravitated with reference to my views." For a fuller statement I must refer to the *Monist*, vol. i. pp. 1–20.

It is quite true, as he says, that "in his original paper, and in the summary of it published in NATURE, Dr. Romanes adduced variations in regard to fertility and sterility as the fundamental fact in physiological selection." And this is exactly what Mr. Wallace himself has done in his "alternative theory." Taking it for granted that these variations must always occur—as in my "original paper" I said they probably "most frequently" occur—in a whole race or strain, his theory seeks to explain, (1) the *causes* of such variations, and (2) their *effects* in furnishing an important condition to the origination of specific types, and this exactly in the manner that the theory of physiological selection had previously exhibited. Space forbids any long quotations, and therefore on the present occasion I will confine myself to transcribing two sentences from the paragraph in which he very correctly summarizes his theory, viz. the sentences which deal with the *effects* of these "variations in regard to fertility and sterility."

"The preceding argument, it will be seen, depends entirely upon the assumption that some amount of infertility characterizes the distinct varieties which are in process of differentiation into species. . . . It is by no means necessary that all varieties should exhibit incipient infertility, but only some varieties; for we know that, of the innumerable varieties that occur, but few become developed into distinct species; and it may be that the absence of infertility, to obviate the effects of intercrossing, is one of the usual causes of their failure."

Here we have the whole essence of "physiological selection" in a nutshell. The first sentence conveys the "fundamental fact;" the second indicates its possibly important consequence in permitting the origination of species by preventing the effects of intercrossing. Why, then, does Mr. Wallace not perceive that this is the whole essence of physiological selection? As far as I can understand, the reason appears to be that he deems his variations in the direction of cross-infertility to differ from mine, in that while his may be associated with other conditions or causes of modification, mine, as he now repeats in NATURE, are supposed always to act "alone in an otherwise undifferentiated species." Now, not only did I expressly guard against this interpretation in my "original paper," but in all my answers to Mr. Wallace's criticisms which have since appeared I have over and over again corrected the mis-statement on his part; and I am the more surprised that he should again reproduce it as the sole basis of his present reply, inasmuch as some of the passages which he quotes for this purpose from the paper in question themselves furnish the needful correction in their own immediate context.[1] Moreover, not only have I thus from the first fully recognized the sundry other causes of specific change with which the physiological variations may be associated; but Mr. Gulick has gone into this side of our common theory much more fully, and elaborately calculated out the high ratio in which the differentiating agency of any of these other causes must be increased when assisted by—*i.e.* associated with—even a moderate degree of the selective fertility, and *vice versâ*. Therefore, it is simply impossible for Mr. Wallace to show that "our theory" differs from his in this respect. Yet it is the only respect in which his reply alleges any difference.

I do indeed myself believe that in many cases the physiological variation may arise alone, in an otherwise undifferentiated species; but from the first I have always maintained that it makes no essential difference to the theory in what proportional number of cases it has done so. And in a forthcoming treatise I shall be able very completely to dispose of Mr. Wallace's "two carefully considered cases," whereby he claims to have proved that the possibility of physiological selection ever working alone is "absolutely unfounded."[2] At present, however, the point is that, even if I am wrong in supposing that physiological selection can ever act alone, the principle of physiological selection, as I have stated it, is not thereby affected. And this principle is, as Mr. Wallace has re-stated it, "that some amount of infertility characterizes the distinct varieties which are in process of differentiation into species"—infertility, whose absence, "to obviate the effects of intercrossing, may be one of the

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[1] For instance, to take only the first of his "few quotations," he reproduces by itself the following sentence:—"It becomes almost impossible to doubt that the primary specific distinction [meaning sterility] is, as a general rule, the primordial distinction." Now this, be it remembered, is quoted for the expressed purpose of "making it absolutely clear that Dr. Romanes's theory of physiological selection, so far as it had any originality, *was founded on the supposition of sterility alone, arising in an otherwise undifferentiated species.*" Yet, if Mr. Wallace had but read the context, he would have seen that this statement is directly contradicted. For the very next sentence is as follows:—"I say as a general rule, because the next point which I wish to present is, that it constitutes no part of my argument to deny that in some—possibly in many—cases *the primary distinction may have been superinduced by the secondary distinctions.*" By the "primary distinction" Mr. Wallace correctly understands me to mean cross-infertility between allied species, while by "secondary distinctions" it is explained in the same place that I mean specific characters of all other kinds. The passage then goes on, through a number of pages, to show "how natural selection, or any other cause [which is concerned in the differentiation of species], may have induced this particular kind of variation in the reproductive system by its operations on other parts of the organism," and the ultimate conclusion of this lengthy argument is: "Thus, we see, it really makes no essential difference to my theory whether it be supposed, in any given case, that the primary distinction was prior, or subsequent to the secondary distinctions." Comment appears needless.

[2] These cases consist in some exceedingly simple arithmetical computations, which, as I shall hereafter show, rest upon erroneous data. Mr. Fletcher Moulton, who has kindly gone into the matter in a really efficient manner from the mathematical point of view, reports, to use his own words, "an enormous difference from Mr. Wallace's results."

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